

## The nomenclature question.

### Some inconsistencies in plant nomenclature.

In a recent unpublished letter a prominent botanist calls attention once more to an argument that has often been made use of by the opponents of the so-called "reform" movement in botanical nomenclature: namely, that a motive, if not indeed the prime motive, for all this upsetting of names is to be found in the desire of the reviser to append his own name to all possible combinations of genera and species; in other words, that the sole end and aim of this nomenclatorial agitation is the theoretical opportunities it gives for incompetent writers to juggle with the names of our plants with the purpose of constituting themselves the authority for as many as possible. As a matter of fact nothing could have been farther from the minds of the nomenclature committee than this feature; and it was largely to obviate just such a possibility that the reform movement originated. By setting an initial date logically fixed at the beginning of binomial nomenclature behind which it is agreed not to go, and referring each species to the oldest subsequent name, the matter becomes fixed for all time. It is unfortunate that it is found necessary to change so many of our plant appellations, but when once so changed in accord with this logical principle, we shall have, it seems to me, a practically stable system of nomenclature. Other departments of biology have long since found it necessary to adopt similar rules, and their experience proves conclusively that it is a reform which reforms. The American ornithologists, for example, have been obliged to make less than one per cent. of corrections during the ten years' application of their code, and not one of these corrections was due to mere personal opinion; the nomenclature of North American birds is therefore practically stable, and I can see no reason why the botanists may not consequently hope for a similar fixation of plant names.

In order to show that the principle is open to criticism which regards the last author of a combination of genus and species as more important than the original namer of the same plant, I take the liberty of citing a number of examples. In the *Synoptical Flora of North America* 1<sup>2</sup>: 397-407, the genus

Cnicus is found to embrace forty-two species and varieties. Of these, Gray is given as authority for no less than thirty-two; but by looking through the synonymy it appears that fifteen of the names, or nearly fifty per cent., had been given previously by other authors, as Nuttall, Muhlenberg, Hooker, Engelmann and De Candolle. Thus *Carduus undulatus* Nutt., 1818, becomes *Cnicus undulatus* Gray, 1874; *Cnicus discolor* Muhl., 1804, becomes *C. altissimus* var. *discolor* Gray, 1883, etc. The same practice may be observed in Watson's treatment of *Lesquerella* elsewhere, in which twenty-four out of thirty-five names credited to him had been previously given by other authors. All right and title of the original discoverer of a species thus disappears.

Another step in the working of this principle is shown in the recently issued fascicle I of volume 1, part 1 of the Synoptical Flora, where the transferred species or varieties are followed by the abbreviations "n. sp." or "n. var." as the case may be. Thus we learn that *Clematis Pitcheri* var. *Bigelovii* is a "n. var." notwithstanding the fact that *C. Bigelovii* was described by Torrey in 1856! *C. Pitcheri* var. *filifera*, another "n. var." was described as *C. filifera* by Bentham in 1848. *C. verticillaris* var. *Columbiana* Gray, n. var. 1895, was described originally by Nuttall in 1834 and was made a "new species" again by Torrey and Gray in 1838. *Eutrema Eschscholtzianum* Robinson, n. sp. 1895, is *Aphragmus Eschscholtzianus* Andr., 1824, while *Braya humilis* Robinson, n. sp. 1895, is *Sisymbrium humile* C. A. Meyer, 1831. It is unnecessary to multiply examples. To my mind it does not seem probable that the practice of placing one's name after a species is likely to be more abused by the advocates of sound nomenclature than it has been in the past by the adherents of conservatism. It has usually been the custom to append "n. sp." or "n. var." only to species or varieties that are described for the first time as new to science, although the same abbreviations have occasionally been used where it has been found necessary to give a new name to a previously described plant, instances of which may be found in this same fascicle of the Synoptical Flora. This is the usage throughout the whole range of biology, without, so far as I can find an exception. If this innovation should ever become general, some other method of designating species and varieties that are really

new will have to be devised, since the old familiar practice will have lost its force.<sup>1</sup>

The facts in the case, it seems, are simply these: The proposition that the author who makes "the first correct combination" of genus and species is entitled to more credit than the original discoverer of that species, cannot be maintained. Upon this point the committee appointed by the British Association to prepare a code of nomenclature makes the following statement:

" . . . We conceive that the author who first describes and names a species which forms the groundwork of later generalizations possesses a higher claim to have his name recorded than he who afterward defines the genus which is found to embrace that species, or who may be the mere accidental means of bringing the generic and specific names into contact. By giving the authority for the specific name in preference to all others, the inquirer is referred directly to the original description, habitat, etc., of the species, and is at the same time reminded of the date of its discovery."

This committee numbered Darwin, Henslow, Wallace, Babington, J. D. Hooker, Balfour and Bentham among its members.

To the statement that preference should be given to the referer of the species to its proper genus on the ground that "it requires greater knowledge of the structure and relationship of species to properly classify them than to simply name and describe them," the code of nomenclature adopted by the American Ornithologists' Union says, "But it often happens that the authority for the combination of names used is not that of the classifier, but of the author who merely 'shuffled names,' or worked out the synonymy in accordance with nomenclatural rules, and has had nothing to do with the correct allocation of the species."

The concurrence of opinion is, therefore, to the effect that the name of the original author of a species is an inseparable part of the specific name, and should go with it no matter what its vicissitudes may be, not only as a matter of simple justice, but from the standpoint of historical accuracy. This so-called "correct combination" is a personal equation and

<sup>1</sup>Since the above was written Prof. Bailey has, unfortunately it seems to me, adopted this innovation and writes "*n. sp.*" after his *Carex Arkansana*, which had been described as *bona fide* new as a variety in 1888.

can never be a fixed quantity. It needs but a glance at our manuals to show that generic and specific limitations are variously understood by writers, and who shall be entitled to say which is the truly "correct" combination? Indeed the authority for the last combination is regarded as of so little importance by American ornithologists that they omit it in writing the names of North American birds. Personally, I prefer the double citation, for then the history of the species becomes complete. The namer of the species and the authority for its present combination both receive the recognition justly due them.—F. H. KNOWLTON.

### Botanical nomenclature.

Perhaps enough has been said on the subject of botanical nomenclature, yet I would like to offer some comments on certain phases of it that have been made prominent by some of the advocates of the Rochester and Madison rules.

It seems to be taken for granted by them that the signers of the Harvard circular were, and are, influenced by considerations of sentiment and prejudice in opposing the so-called reform in botanical nomenclature, whereas the contrary is the truth.

To assert that such men as Dr. Farlow, Prof. Eaton, Dr. Goodale and Dr. Robinson, and I might very properly add Dr. Gray and Sereno Watson who when living were in sympathy with the spirit which subsequently found expression in the Harvard Circular, would permit themselves to be influenced by mere prejudice and sentiment in such a matter is quite as discreditable to those eminent botanists as it is to those who make the assertion. Rather it is that the signers of that Circular believe with the late Prof. Eaton, who wrote me to this effect only a short time before his fatal illness, that the proposed methods of reform, so-called, would tend to increase rather than to diminish confusion.

By far the ablest paper, the fairest and most courteous that has yet appeared in defense of the new rules is that published by Lester F. Ward in the *Bulletin* of the Torrey Botanical Club in July, 1895, yet Mr. Ward certainly errs in assuming that the signers of the Harvard Circular are influenced by mere sentiment and prejudice, or a "personal disinclination to incur the annoyance of accustoming themselves to a new set of names." Among those signers of whom I have knowl-